

AUTHORS' REPLY

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The tenor of Lee's discussion does not really justify a debate, particularly when it is based on 42 references, 37 of which come from his team. We suspect that some of the points raised by Lee are almost exclusively included as pretexts to present their extensive bibliography. However, we provide here some general comments addressed to end users of our equations and separately in the appendix we offer more detailed discussion on the nine points raised by Lee which will be of interest mainly to researchers in engineering seismology.

We must stress at the outset that our paper¹ is concerned only with the attenuation of absolute acceleration spectral ordinates and peak ground acceleration and discussion regarding other strong-motion aspects is not particularly relevant.

The equations that we have presented enable reliable estimates of zero-period and spectral accelerations to be obtained in seismic hazard assessments in Europe on the basis of parameters that can be quantified for future earthquakes with relative ease. The desire to publish our results stems from our concern regarding the widespread use in Europe of standard spectral shapes to construct design spectra and the use of frequency-dependent attenuation relations which are based either on unnecessarily limited datasets or else on data from outside the Eurasian region. The important point is that no attenuation equations for this whole region have been derived previously.

We have used all the European strong-motion data currently available to us without applying any exclusion criteria other than an unacceptable quality of digital data. The predictive relations that we have derived are robust and the scatter associated with our equations is not higher than that usually found in attenuation studies.

We did refer in our work to four other attenuation studies for parts of Europe (two for Italy, one for Greece and one for the Eastern Mediterranean) and it was perhaps an oversight on our part not to make reference to the work of Lee² on strong-motion attenuation in Yugoslavia nor to the work of Caillot and Bard³ for Italy. We can find no significant variations among regions and doubt whether the discrepancies between our results and those of Lee² are genuinely due to regional differences rather than other factors such as the methodology of analysis and the distributions of the datasets.

On other points we do not feel that in the present case a lengthy reply is particularly useful, but some brief comments are offered for consideration. Regarding the proposal to apply an instrument correction to all records using standard dynamic characteristics, Lee himself points out that the influence is likely to be very small in the period range we consider. On the issue of the period range, we have deliberately limited ourselves to a maximum period of 2.0 s, as have Boore *et al.*⁴ and Lee.² We do not have great confidence in the data at longer periods and it seems that nor does Lee² who states that the Yugoslavian data '*beyond $T > 2.0$ s is not adequate for meaningful regression at this time*'.

Lee challenges us to employ more complex characterization of the local geology at the recording sites through the use of two separate terms as he and his co-workers have done in several of their studies. This may well represent a desirable improvement to the model but at the present time sufficient data is simply not available and the choice is essentially to derive relatively straightforward attenuation equations for the engineer as we have done, or to adopt a more complex model whereby the usable dataset would become so small as to be unreliable for regression analysis. In such cases over-parameterization is not an improvement

of the attenuation model. Some authors seems to have paid little attention to this and their over-estimation of the importance of using too many parameters has led to a widespread belief that their relations are verified to a degree that their input data do not in the least warrant.

We are also bemused that people still wish to use intensity to predict strong-motion parameters and develop attenuation equations that include this response variable as one of the predictor variables.

In what follows we address all of the points raised in order to reconfirm our confidence in the equations that we have provided for the engineering community in Europe and surrounding regions.

APPENDIX

The numbers in parentheses refer to the points raised by Lee in his discussion.

(1) We appreciate that different regions may have different attenuation characteristics, although we believe these regions should be defined by physical rather than political boundaries. Lee bases his arguments on apparent regional differences in the attenuation of intensity. However, there is no doubt that the attenuation of intensity is regionally biased rather than regionally dependent. This is not so much because of regional differences in the geological or seismological environment that may actually exist between different geographical regions but rather because of cultural differences in the predominant building stock, in the intensity scales and in the way they are interpreted, as well as in the way isoseismal maps are drawn. This bias is not only regional but also time dependent and changes or revisions of attenuation relations often coincide with changes in the personnel responsible for the production of national intensity maps or when more than one agency is involved in their preparation.⁵ In most of the attempts made to correlate intensity with acceleration the enormous scatter of the data clearly shows, as it should, that there is no physical justification for a one-to-one relationship. It is rather disappointing and rather out of date for Lee to bring in intensity considerations in order to justify the need for country-specific attenuation relations. We prefer not to make assumptions that are not testable and to advocate on the basis of inadequate physical evidence.

It is not clear to us why Lee and his colleagues limit themselves exclusively to data from Yugoslavia. We have found for peak accelerations remarkable agreement between Europe and western North America and we are as yet unconvinced by apparent regional differences such as are found in Central America and Japan. Ideally, individual tectonic regions should have their own relationships but this at present is impossible because of the limited availability of data. Comparison of data subsets from the full European dataset shows that regional (country) differences are not very large, certainly for near-field predictions (Figure 1). Some of the differences no doubt arise from the limited size of the subsets of data and their different distributions and biases. However, one should not give too much importance to these trends since there is little physical basis for the groupings within political boundaries, which seismologists need to be able to look beyond. Several of the 'Yugoslavian' accelerograms are recordings of the Friuli earthquakes of 1976–1977 just the other side of the border with Italy.

Sophistication of the model by the inclusion of additional independent parameters such as the source dimension and coherence length that Lee suggests might be desirable if the dataset were adequate to determine reliably their influence on the strong motion. However, even if this were the case, this then places the onus on the engineer to assess *a priori* parameters which are difficult enough to calculate for previous earthquakes.

We are well aware that an attenuation law of the form r^n is not a perfect representation of attenuation with distance. Ideally, the relation should be of the form $e^{kr} \cdot r^n$ with the values of k and n varying with distance in response to changes in the contributory strong-motion wave types. However, as stated in the original paper, simultaneous solution of both distance parameters was not possible due, we believe, to the distribution of the dataset. Also, at least according to Joyner and Boore⁶ and references quoted by them, most strong motion to distances up to 100 km can be adequately described in terms of S -waves, and beyond 100 km in terms of L_g phases. For the geometric spreading function (n), values of n in a uniform medium theoretically are -1.0 for S -waves and $-5/6$ for L_g waves. Surface waves, again according to Joyner and Boore,⁶ are generally only important at longer periods (> 3.0 s), thus outside the range of our relations and therefore have no bearing on

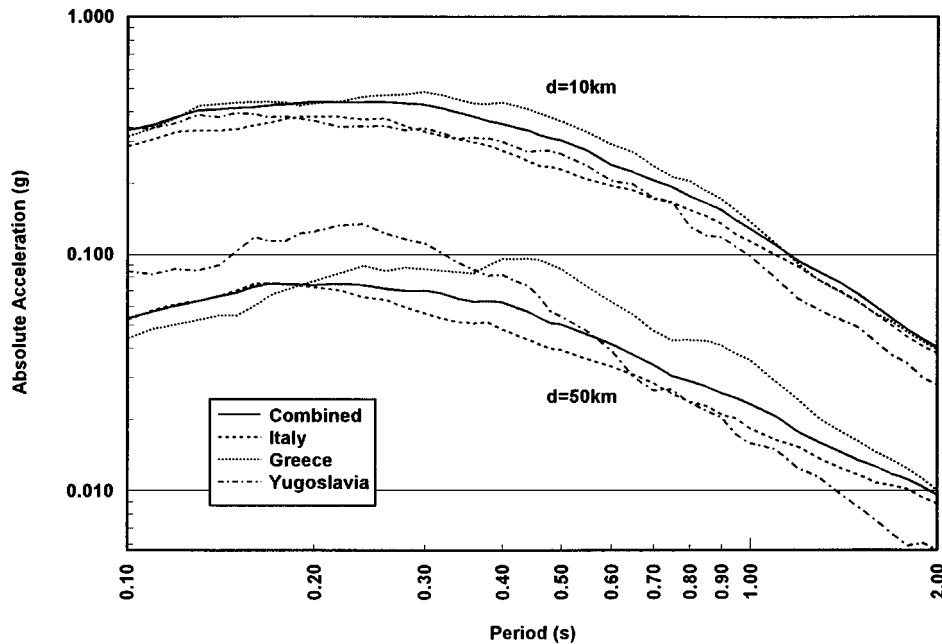


Figure 1. Horizontal absolute acceleration response spectra at 5 per cent of critical damping for a magnitude $M_s = 6.0$ earthquake at distances of $d = 10$ and 50 km, using the full European data set compared to the spectra obtained using subsets from Italy, Greece and Yugoslavia, independent of site effects.

the choice of n values. The important point again is that since the dataset distribution is not sufficiently good to permit the simultaneous determination of reliable estimates of geometrical and anelastic attenuation, it is clearly not going to be possible to determine meaningful values for a more sophisticated model.

Beyond this we feel unable to comment on the uniformity and quality of their strong-motion dataset because in the references quoted in the comments only a list of causative earthquakes is given. Their full Yugoslavian dataset, with distances, soil conditions and peak accelerations, has only been published in an internal report of the University of Southern California⁷ and strenuous efforts on our part to obtain this document have met with equally strenuous efforts to keep it from us. We feel that all strong-motion attenuation studies should present the datasets on which they are based in full so that others can corroborate the results and we would like to see an end to 'proof by inaccessible reference'.

(2) As already stated in the original paper, the majority of our data comes from SMA-1 instruments with a nominal natural frequency of about 25 Hz and therefore instrument effects are not likely to be of concern to our relations. The use of nominal values is very questionable since we know of several cases in our dataset where instruments have been recalibrated in the field and found to have frequencies different from the original calibration. Moreover, for a limited number of the records we do not know which type of instrument produced the accelerogram.

On the question of the filter applied, we believe that the choice of an ideal filter is unresolved and likely to remain so indefinitely. In our work we routinely use the elliptical filter which produces reliable results; this does involve time reversal of the record in order to eliminate phase distortion. While there may be relevant advantages and disadvantages of this procedure in comparison to other filters, there is no consensus amongst engineering seismologists to suggest that this is in any way '*not acceptable*' as Lee seems to think.

(3) The argument concerning the use of terminology is not trivial and highlights the lack of a unified classification system for characterizing 'foundation material' in engineering seismology. Complications arise because the engineering community is still to develop a comprehensive understanding of the effects of foundation material on strong ground-motion. Consequently characterization approaches developed over

two decades ago can hardly be expected to be maintained unchanged. By the reasoning of Lee the terms 'rock', 'stiff soil', 'deep soil' and 'soft to medium soil' should never have been used in any other form except that defined by Seed *et al.*⁸ In practice these definitions have been changed by others that have adopted this scheme^{9–11} the changes especially involving reclassification of deposit depths for 'stiff' and 'deep' soil, but furthermore 'stiff soil' has been considered in terms of deposits < 200 ft/60 m and deposits > 200 ft/60 m. Thus use of the term 'stiff soil' is not as Lee states restricted to shallower depths, even by one of the authors of the original classification.¹⁰

Elsewhere Crouse¹² considers 'stiff soil' sites with depths < 25 m. Others^{13–16} use the classification of Sabetta and Pugliese¹⁷ which considers a 'stiff' material and 'deep soil'; 'stiff' sites being defined as material having an average shear-wave velocity > 800 m/s but includes sites with less than 5 m soil (velocity 400–800 m/s); 'deep soil' being sites of > 20 m soil of velocity 400–800 m/s. Borchardt^{18,19} employs a classification which includes 'stiff' clay and sandy soil defined by a velocity of 200–375 m/s and > 5 m thick. Similarly numerous variants for definitions of 'rock' and 'soft soil' are in existence.

A further point to note is that terms currently in use have been borrowed from geotechnical engineering, engineering geology and geology and applied in a different context from that for which they were originally defined. The use by Lee of the terms '*soil conditions*' and '*site geology*' to imply a scaling effect between foundation materials less than a few hundred meters thick and those up to several kilometers thick is not employed universally. In fact Trifunac and Brady²⁰ make no such distinction in their discussion. The majority of workers have like ourselves, also used these terms in an interchangeable form within the general category of site response.^{6, 21–23} Others have used different approaches, for example, Rogers *et al.*²⁴ consider foundation material effects in terms of 'geotechnical' and 'geological' factors, here geological factors including the influence of shallower deposits (< 100 m), intermediate to deep deposits (< 3000 m) and deep to very deep (< 12 km) deposits.

In light of the previous comment concerning the application of terms in a different context from which they are derived, it can be argued that use of the terms '*soil conditions*' and '*site geology*' are misleading and even '*erroneous*'. The terms 'soil' and 'rock' refer to particular groups of materials with defined physical properties, consequently reference to the effects of '*soil conditions*' should exclude discussion on the effects of 'rock'. 'Geology' is a very broad term encompassing all facets of rock and soil type, as well as structural features (bedding, jointing, faulting), mineralogical features, weathering effects, etc. which are not considered in the characterization scheme. Furthermore, the ability to describe a site with a (deep) '*site geology*' of '*sediments*' and with a (shallow) '*soil condition*' of '*rock*' seems ill-chosen and confusing.

It is very clear we make no distinction between near-surface foundation materials ('*soil conditions*') and deep foundation materials ('*site geology*'). However, Lee overstates the possible importance of the deep materials, other studies finding that this is only important at longer periods. Joyner and Fumal²⁵ investigated the influence of the depth to rock for 'sediment' depths up to 1 km. They concluded that it is only the top 100–200 m of material that is important in determining local site response. Rogers *et al.*²⁴ considered possible factors effecting Fourier spectral ratios in three spectral bands: short period (0.2–0.5 s), intermediate period (0.5–3.3 s) and long period (3.3–10.0 s). For the short periods they found the two most important factors were the depth of shallow Holocene deposits (< 75 m) and the void ratio (and hence shear-wave velocity). For intermediate and long periods the two most important parameters were the depth of Quaternary deposits (< 3 km) and the depth to crystalline basement (< 12 km). However, for the intermediate band, spectral ratios only showed variation at sites with approximately < 100 m depth of Quaternary or to basement, whilst for greater depths ratios remained unchanged. For the long period band spectral ratios are influenced at sites with Quaternary or basement depths up to ~4 km.

Lee states that it is a simple and easy task to describe sites in terms of the '*site geology*' classification, '*sediments*' ($s = 0$), '*intermediate*' ($s = 1$), and '*basement rock*' ($s = 2$). In fact, Trifunac and Brady²⁰ noted the difficulty of this scheme. They used 8 '*experts*' to each define their sites and found that what is meant by '*base rock*' or '*deep alluvium*' varied widely from one '*expert*' to another with individual sites, particularly '*sediment*' and '*intermediate*' sites assigned s values of 0, 1 and 2. They also pointed out: '*... we did not make an attempt to describe our site classification in detail and precisely. We believe that it is virtually impossible to do this unequivocally and to satisfy all important constraints at the same time.*'²⁰

(4) It is not surprising that the A, B, C, D classification of Boore *et al.*⁴ is statistically insignificant when used simultaneously with the ‘local soil’ classification of Seed *et al.*⁸ and the ‘site geology’ of Trifunac and Brady.²⁰ The A, B, C, D scheme is intended as an alternative ‘local soil’ approach, not a supplement. Without seeing the particular reference quoted by Lee, it is hard to speculate, but we wonder how the regression was performed to obtain the relevant coefficients.

For the European relations presented in the paper, the derived coefficients are statistically significant at the 95 per cent confidence level.

(5) The purpose of the $M_0 - M_S$ discussion in our paper is clear, although possibly overstated. The joint use of \mathbf{M} and M_S is not appropriate for strong-motion prediction and hazard assessment unless account is made of the non-linear relationship between the two parameters. This is particularly important in Europe for which few \mathbf{M} values are available, yet where practising engineers may employ such values together with M_S magnitudes without knowledge of the consequences.

For clarity readers of the discussion should be made aware that the ‘magnitude, M ’ discussed by Lee is not the moment magnitude, \mathbf{M} , but refers to teleseismic and local strong-motion magnitudes, thus as Lee notes a similar divergence is seen to that observed for our $M_S - M_0$ relation. However, Lee states that the slope of M against M_0 decreases for $M < 5.5$. Inspection of the quoted reference [Fig. 3 in Reference 20] shows this is not the case. This figure is plotted in terms of M_0 against M for which the slope does decrease for $M < 5.5$; however, inverting the figure to that of M against M_0 shows the same trend as that of Figure 2 of our paper.

The assumption from the last section of paragraph 1 of point (5) of Lee’s discussion is that he believes it inappropriate to define a single $M_S - M_0$ European relation in the same manner as it is inappropriate to employ a single European strong-motion relation (point 1). We, however, find no differences for crustal earthquakes in the seismically active part of Europe.

Lee has missed the point that our main reasons for using M_S are that it is the only scale on which we have uniform and reliable determinations for events covering a wide range of magnitudes and that it is the scale most widely used in the assessment of seismicity in Europe.

Local magnitudes determined from strong-motion records are always associated with much higher standard deviations than teleseismic magnitudes. Furthermore, the correlation of strong-motion parameters with M_L^{SM} is not strictly appropriate since they are not genuinely independent parameters.

We are also puzzled by Lee’s insistence on the one hand for use of magnitude scales based on high-frequency radiation and on the other hand for the extension of the equations to long periods.

(6) The arguments for and against saturation of strong-motion amplitudes with magnitudes greater than about 6.5 are unresolved and it is curious that Lee defends the premise by reference exclusively to his own work. We did consider the inclusion of a quadratic term in magnitude in our equations, but in view of the quadratic relation between M_S and seismic moment, our linear scaling of surface-wave magnitude is actually equivalent to the quadratic scaling of moment magnitude employed for example by Boore *et al.*⁴

(7) It is unknown how site-dependent residual trends are incorporated into the Californian relations of Lee *et al.*²⁷ and Lee and Trifunac²⁸ as these studies have not been seen. However, for the European data modification of the existing predictive relation is not necessarily straightforward. The use of an arbitrary function which removes the observed trend is not appropriate since it is found that the observed trends are not consistent for some strong-motion stations of the same site category. Some stations show strong trends in the same form as the overall solution whilst others can show the opposite relationship. This is a matter of ongoing work and we would rather produce a solution which is consistent for all data than introduce some arbitrary non-physical function which simply accommodates the residual trends.

(8) The comparisons made between spectra predicted by our equations and those from the equations based on Yugoslavian data are not entirely valid for two important reasons: the distance we have used is measured from the surface projection of the fault rupture and is not epicentral (the continued use of epicentral distance puzzles us) and the ‘correction’ — by which we assume Lee means ‘conversion’ or ‘adjustment’ — to our values of M_S , through a relation whose origin is not specified.

(9) Apart from the question of the period range, which we have answered in the main text, it is not clear to us what point Lee is trying to make here, since the issue of estimating magnitude–frequency relations is utterly irrelevant to the issues at hand. Furthermore, our equations are consistently presented with a clear

measure of the residuals based on the usual assumption of a log-normal distribution. Our equations are perfectly suited to incorporation into probabilistic seismic hazard assessments, which may not be true for equations involving parameters related to the path geology.

We welcome this opportunity to expand on some of these issues that space did not allow us to include in our paper. However, further exploration of these issues would turn our response into another paper in its own right. This would clearly not be appropriate, particularly in view of the unknown motives for Lee's precipitated attempt to discredit our study which is an important contribution to seismic design practice in Europe.

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